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Interactive comment on "Temporal variability of chlorophyll distribution in the Gulf of Mexico: bio-optical data from profiling floats" *by* Orens Pasqueron de Fommervault et al.

Orens Pasqueron de Fommervault et al.

orens@cicese.mx

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Please find below our reply to comments to our submitted manuscript from reviewer #2. The numbering of lines or pages refers to the submitted version of the manuscript.

(Reviewer) The manuscript Temporal variability of chlorophyll distribution in the Gulf of Mexico: bio-optical data from profiling floats by Pasqueron De Fommervault et al. attempts to evaluate the temporal and spatial variability of chlorophyll concentration in the Gulf of Mexico. The study utilises data from eight bio-optical profiling floats. The paper addresses the winter increase in sea surface chlorophyll concentration and the impact of mesoscale eddies on phytoplankton biomass in the Gulf of Mexico. This is

done by applying already published methods to a rather new dataset. The problem is that, most of the time, the methods cannot be applied or are not applied correctly. Consequently, most of the results presented in the manuscript are flawed. Additionally, the manuscript is poorly written, the arguments are hard to follow, and often not justified by the appropriate references. The amount of work needed for publication in Biogeosciences is considerably more than for a major revision.

(Authors' response) We thank Referee #2 for his/her review and comments. The major criticisms are related to methodological issues that prevent, according to the reviewer, an accurate analysis of our dataset. Below, we address and discuss all the comments hoping they will clarify our work and answer the reviewers' concerns. Additional references and figures are also presented. The comment that the manuscript is poorly written and the arguments hard to follow is difficult to address since no specifics are provided. We note however, that reviewer #1 considers that the paper is "well written" and that it "flows well" (even if he/she has other concerns that we have addressed in the corresponding reply).

(Reviewer's comment 1) One of the conclusion of the manuscript is that the winter increase in sea surface chlorophyll concentration is due a photoacclimation process or a reentrainment of phytoplankton cells at depths. I felt like the authors have chosen to copout on testing one hypothesis over another. The authors have all the data necessary to investigate what causes the winter increase in sea surface chlorophyll concentration Gulf of Mexico. They need to do more than just commenting on the float observations.

(Authors' response) In the manuscript it is suggested that the winter surface chlorophyll increase could result from (i) photoacclimation within the mixed layer (ML) due to a reduction in average light availability, and/or (ii) from mixing of the deep chlorophyll maximum (DCM) into the ML. Processes involved in (i) are considered on a seasonal basis. The hypothesis is that during the winter mixing period, the average light intensity for phytoplankton is reduced in the ML with respect to summer conditions leading to an increase of intracellular chlorophyll content. Since, on average, a low bbp/[chl] ratio at the surface is persistent in winter, this hypothesis seems reasonable. Hypothesis (ii) supposes that low-light acclimated cells from the DCM are transported to the surface by mixing process, thus increasing the bbp/[chl] ratio at the surface. Considering only (ii) assumes that phytoplankton cells do not have time to re-acclimate to their new light environment (hours to days processes). So it is not warranted to test (ii), given the temporal resolution of our floats (two profiles per month from each float). Contrary to the reviewer's claim, we do not think the data have enough temporal resolution to determine what causes the winter increase in surface chlorophyll. That is the reason why we are cautious and suggest that is difficult to attribute the observed changes in chlorophyll to only one mechanism and suggest/speculate possible mechanisms. Unless the reviewer can point towards specific references or analysis tools that could help resolve this question with the temporal resolution and parameters measured by the floats, we stand by our original conclusion that this cannot be done with the data available.

(Reviewer's comment 2) The authors use the depth of the 6C isotherm to classify the eddies in the Gulf of Mexico. They argued that this isotherm has a mean depth of 795 m and that Bunge et al (2002) found that this isotherm separates the deep stable water from the eddy-influenced surface water. Finally, it is said that Hamilton et al. 2017 found a strong correlation between the isotherm and upper layer eddies. First, Bunge et al. (2002) did not identify eddies with this isotherm. It was used to delineate the depth of the Loop Current in the Yucatan channel. Consequently, the use of the 6C isotherm to identify mesoscale eddies cannot be justify by this reference. Second, the vertical extent of eddies core is comprised, in average, between 300 and 400 m. What is the rationale of using such a deep isotherm to detect eddies that impact the first 400m of the water column? Third, the authors claimed that Hamilton et al. (2017) found a correlation between mesoscale eddies and the 6C isotherm. However, this reference seems to be an oral presentation and again it cannot be used to justify the utilization of the 6C isotherm.

(Authors' response) First, note that the analysis is meant to detect mesoscale structures (i.e nearly geostrophic flow structures that can be identified from sea surface height anomalies / thermocline depth anomalies) but also structures that are part of the mean circulation. In the Gulf of Mexico (GOM), besides transient eddies such as the anticyclones detached from the Loop Current (LC), the cyclones that often accompany the detachment process and other eddies present inside the GOM, there are semi-permanent structures that, for example, can be seen in the figure below (Figure 1) which shows the mean dynamic topography of the GOM (Rio and Hernandez, 2004, and mean-dynamic topography.html). One can see that the LC itself (anticyclonic), the cyclonic gyre of the Bay of Campeche and the (anticyclonic) eddy/gyre straddling the central GOM (produced in part by the path of LC eddies inside the GOM) are features of the mean circulation.

Hence, to avoid misunderstanding and to make sure it is clear we are referring to mesoscale structures and not only "eddies" which are generally understood as transient and closed circulation features, the term eddy in page 5, line 27 is now replaced by mesoscale, and removed page 9 line 16.

Second, in several references (e.g. Donohue et al., 2007; Hamilton 2007; Sheinbaum et al., 2007; Donohue et al., 2008; Hamilton et al., 2016) it is shown that the vertical displacement of the 6 to 10°C isotherms (located at the base of the LC and LC eddies) is anti-correlated with sea-surface height displacements, highlighting the importance of first baroclinic mode dynamics in the GOM. 6-7°C isotherms are at 450-500 m depth within some cyclonic eddies. Donohue et al., (2008), Cox et al., (2010) and Hamilton et al., (2016) suggest the 6°C isotherm is a better choice for a two-layer analysis, and Kolodziejczyk et al., (2012) find the zero crossing of the first baroclinic mode near 1000 m depth.

We agree with the reviewer that not enough references or explanations for using the 6°C isotherm depth (T6) variability used to identify mesoscale structures were given in the submitted manuscript. Thus, To make a stronger case for the use of T6 to identify mesoscale structures, new references are added and list items of pages 5 (lines 30-31) and page 6 (lines 1-4) are re-written:

- The GOM can be, to some extent, be studied as a two-layer baroclinic system, with vertical displacements of the 6 to 10°C isotherms (located at the base of the Loop Current and Loop Current eddies and at 400-600 m depth in cyclonic eddies) being anti-correlated with sea-surface height (Donohue et al., 2007; Hamilton 2007; Sheinbaum et al., 2007; Donohue et al., 2008; Hamilton et al., 2016, Hamilton et al., 2017).

- T6 is in the lower thermocline and has been used as the interface for a two layer system, separating deep waters from the more energetic upper layer containing mesoscale structures and the Loop Current (Bunge et al., 2002; Donohue et al. 2008; Hamilton et al. 2016, Hamilton et al., 2017).

Third, the depth of T6 is directly measured by the floats. Hence we decided to use this parameter to track mesoscale structures instead of sea surface height (SSH) measured by satellites, which have limited spatial resolution so the required interpolation to the locations of our profiles would add additional errors (as it is shown in the manuscript, using SSH instead of T6 leads to essentially the same results, figure S1 in supplementary material).

Finally, note that the baroclinic signal of the mesoscale structures reaches down below 600m (or more) in the GOM (e.g. Lewis et al., 1989; Donohue 2016a and b, Sheinbaum et al., 2007, Hamilton et al., 2017), even if the eddies' core is shallower. Additionally, Hamilton et al., (2017) reference given in the manuscript is not an oral presentation (it is a BOEM-MMS technical report which is (now) available online (https://www.boem.gov/ESPIS/5/5583.pdf). The link will be included in the reference list.

Given the above, we decided to leave the analysis as is, using the T6 criteria to obtain the anticyclonic/cyclonic subsets. However, we can certainly change all the analysis using SSH instead, if the reviewer and editor consider that is a better choice. If so, we

do ask for additional time to recalculate this to include the corresponding new figures, table 2 as well as the values and statistical significance analysis discussed in the text.

(Reviewer's comment 3) The section on the impact of mesoscale eddies on phytoplankton biomass is largely inspired by the study of Dufois et al. 2014. The authors compared the float observations when the floats were profiling in a cyclonic structure with the observations when the floats were profiling in an anticyclonic structure. By averaging observations that were collected at different times of the year and locations in the Gulf of Mexico, you are looking at signals that are both influenced by the seasonal and large scale variability and the mesoscale activity; this is not correct. To properly assess the impact of eddy signal on a given variable, one needs to look at the departure from the seasonal mean (see Cushman-Rosin, 1994). All sections about the role of mesoscale structures need to be changed.

(Authors' response) We did not "average observations that were collected at different times of the year and locations in the Gulf of Mexico" as the referee writes, since we did not average different times of the year. We clustered the data in different groups based on identifying whether the profiles were taken in anticyclonic, cyclonic or neutral vorticity anomalies, following the criteria explained in the text (see also comment 2 above), and then simply computed monthly climatologies for the anticyclonic and cyclonic subsets, gathering all profiles corresponding to each month regardless of the year and the geographic position, and looked for differences between these two clusters. Data are evenly distributed so each month of the year has a similar number of profiles (figure 1, right panel in the manuscript). One may question the representativeness of this climatology given the amount of data (two vertical profiles per month during 5 years from seven APEX floats), but it is what is available now and sufficient, in our view, to make this computation worthwhile.

It is well known that the seasonal cycle of geophysical and biogeochemical variables is not monochromatic and has time scales that overlap those of mesoscale turbulence. Both processes are thus impossible to separate properly, "even by defining running variances over 6-month windows or by subtracting a mean seasonal cycle from the time series before variance computations" (Penduff et al., 2004). So a decomposition of a variable as a time-mean+seasonal+"eddy" is not necessarily warranted as a means to separate seasonal and mesoscale eddy signals. Besides, depending on how calculations are made, the "eddy" part will include lower frequency variations too. Although there is certainly seasonal variability in the GOM related to atmospheric forcing (air-sea interaction river run-off), spectra (e.g. SSH) shows comparable or more energetic variations related to the mesoscale, in a band of frequencies that overlaps with seasonal variations (3-4 months, Jouanno et al, 2016; Hamilton and Lee, 2005).

Having said that, we followed the suggestion of the reviewer to first remove the seasonal signal to try to separate it from the mesoscale variability. We first calculate the climatology for the variable in question using all profiles regardless of vorticity anomaly, and then substract the monthly mean value from each profile to compute the anomalies. These anomalies have now mesoscale as well as lower frequency (interannual) signals. The profiles are then separated in the two clusters (cyclonic and anticyclonic), and a monhtly climatology of the anomalies is obtained. Figures below (Figures 2 and 3), same as figure 5 and figure 7 in the manuscript but for anomalies, show the same results as those obtained from the original calculation. Namely that anticyclones tend to show a deeper mixed layer depth (MLD) than cyclones in winter, that cyclones have slighlty larger integrated chlorophyll content than anticyclones, and that the nitracline depth and steepness is larger for cyclones throughout the year.

We decided not to use the anomalies, given that they are only useful to show relative differences between the two groups (anticyclonic vs cyclonic) of a given parameter, but it becomes complicated when two parameters are to be compared (e.g. the depth of the ML with that of the DCM). We think the discussion is clearer if we stick to the absolute values, and given the results above, we hope that the reviewer agrees with us. However, the title of the section will be changed:

"Impact of mesoscale structures on the annual cycle" Instead of "Role of mesoscale

structures in shaping the annual cycle"

and we will add this paragraph to the corresponding section so as to make a more convincing case for our approach (page 8 line 20):

"It is well known that the seasonal cycle of geophysical and biogeochemical variables is not monochromatic and has time scales that overlap those of mesoscale turbulence (e.g. Penduff et al., 2004) thus, it is not possible to separate them properly. Although there is certainly seasonal variability in the Gulf of Mexico related to atmospheric forcing (air-sea fluxes and river run-off), spectra shows comparable or more energetic variations related to the mesoscale in a band of frequencies that overlaps with seasonal variations (3-4 months for sea surface height, e.g., Jouanno et al, 2016, Hamilton and Lee, 2005). Hence, it could be expected that the variability observed at a given period of the year also depends on the presence of mesoscale structures. We therefore analyzed the seasonal cycle gathering data on a monthly-basis and considering separately profiles acquired in cyclonic and anticylonic structures, to assess the differences between the two groups."

(Reviewer's comment 4) The depth of an isopycnal surface cannot be used to determine a nitracline depth. In the open ocean, nutrient concentrations are controlled by both physical processes such as vertical/horizontal advection and diffusion, convection, and biological processes such as phytoplankton growth, remineralization, etc: : : At a pinch, Eq. (2) can be used to give a crude estimation of the nitracline depth for a quasi-1D steady state system with a surface layer depleted in nutrients, with no change in solar radiation and MLD. In your case, these assumptions do not hold. Sections 3.3.3 and 3.3.3 need to be removed and the conclusion need to be changed accordingly.

(Authors' response) We fully agree that the nitracline depth is controlled by both physical and biological (i.e. depth-dependent) processes. However, actual XIXIMI bottle measurements (obtained both in summer and winter) indicate that, overall, nutrient and density vertical distribution are highly correlated (figure 6 in the submitted manuscript).

This is not something we hypothesize or suggest, nor is it new, it is what the observations indicate. Such a co-orientation of nitrate with density is not surprising, because dynamical processes that vertically displace water masses with their properties are generally strong, compared to the biological pump (Ascani et al., 2013; Omand et al., 2013 and 2015; While et al., 2010). Thus, we considered that for the time scales addressed in our analysis (i.e. seasonal), the methodology provides relevant information in the absence of direct nutrient measurements.

This methodology was applied to profiling float data by Omand et al., (2015) near the station BATS, a region characterized by the passage of mesoscale eddies and winter storms. They showed that the overall nitrate-density relationship varied relatively little throughout the year, and that the "Fluctuation in the nitracline depth due to deep mixing and mesoscale eddies (. . .) though apparent in depth coordinates (. . .) are not distinguishable in density coordinates". Note that a similar approach using an isotherm was also previously applied in the GOM to interpret the seasonal variability of surface chlorophyll (Jolliff et al., 2008). Additionally, if we look at the data at the monthly climatological scale (i.e. not profile-to-profile), the summer nitracline is generally found deeper than the winter MLD, which supports the hypothesis that large input of nutrients to the photic layer by winter mixing are likely limited at these time scales. For the above-mentioned reasons we do not consider that sections 3.3.2 and 3.3.3 need to be removed. However and as also suggested by reviewer #1 sections 3.3.2 and 3.3.3 were rewritten to make the arguments easier to follow (see our response to reviewer 1).

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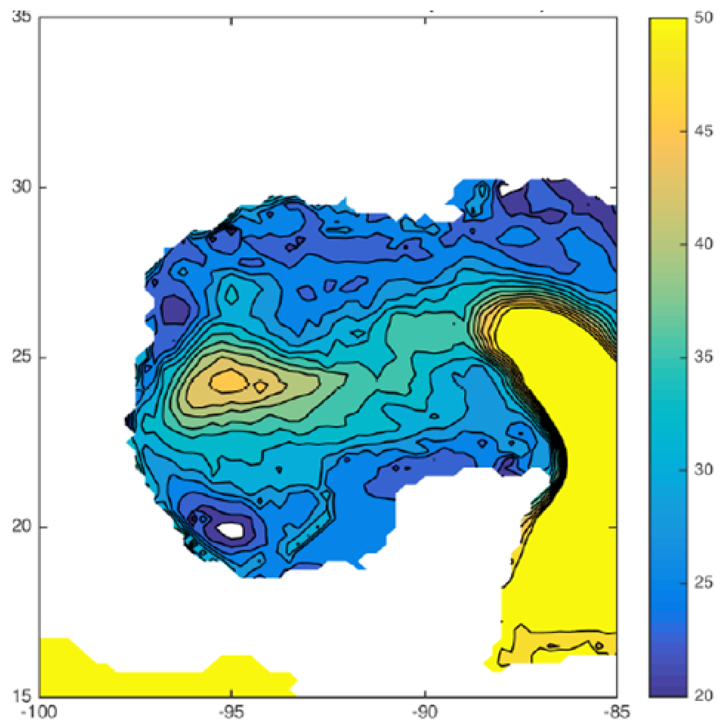

Fig. 1. Figure 1. Mean Dynamics topography in the Gulf of Mexico (RIO-AVISO)

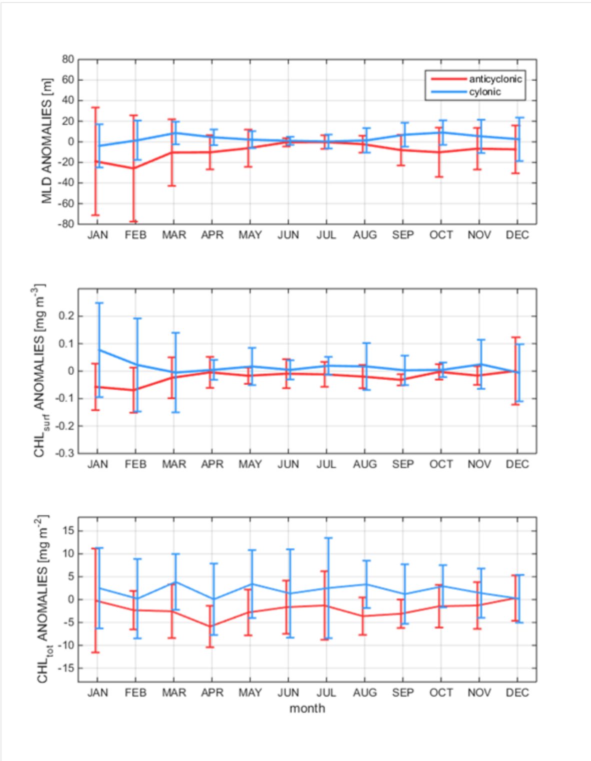

Fig. 2. Figure 2. Same as fig. 5 in the submitted manuscript but for anomalies: (top) the mixed layer depth, (middle) surface chlorophyll and (bottom) integrated chlorophyll over the first 350m.

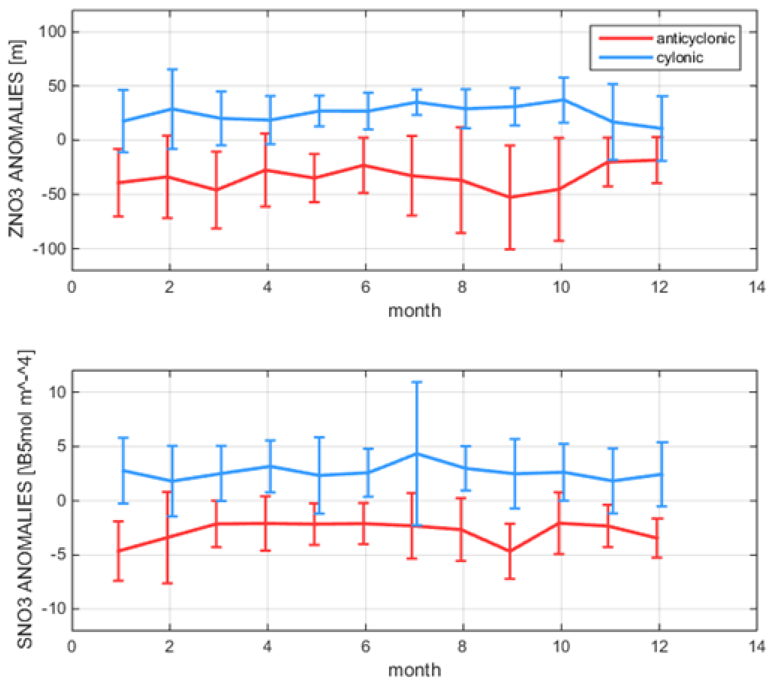

Fig. 3. Figure 3. Same as fig. 7 in the submitted manuscript but for anomalies: (top) the mixed layer depth, (middle) surface chlorophyll and (bottom) integrated chlorophyll over the first 350m.